

SCIENCE

FRIDAY, DECEMBER 16, 1887.

THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH evidently finds difficulty in securing haunted houses to be submitted to their searching investigations. Professor Royce, who is the chairman of the committee on apparitions and haunted houses, jocularly referred to this difficulty in his report at the recent meeting of the society in Boston. As Professor Royce said, the name, suggesting as it does that the time of the committee is mainly spent in visiting haunted houses and ghost-ridden graveyards, does not describe its actual office. The committee has often expressed its willingness to visit haunted houses, or to pass the night in any promising place, for the sake of seeing, explaining, or of converting from the error of its ways, any genuine ghost in the city or in the neighborhood of Boston. The committee has heard of several houses that once were believed to be haunted, but in no case has the present condition of these houses warranted any interference on the committee's part. The phenomena have in all cases so far reported ceased for some time, usually for many years. A more hopeful field is in the direction of tracing some coincidence between a dream or presentiment and its supposed verification by events afterwards, but even in this direction the results are so scattering as hardly to justify the belief in any special significance in the few coincidences which have been traced.

AMERICAN PUBLIC HEALTH ASSOCIATION.¹

ONE of the most original papers presented at the recent meeting of the American Public Health Association was by Dr. E. M. Hunt, secretary of the New Jersey State Board of Health, on the origin of some diseases. It was as follows:—

[PAPER BY DR. E. M. HUNT.]

The class of diseases variously known as contagious, transmissible, or communicable has ever attracted the most earnest consideration of medical men and of all sanitarians. By the quickness and often obscurity of their invasion, by the malignant type they too often exhibit, and by the large areas over which they extend, they not only make large demands upon the skill of the profession, but arrest the attention of all mankind.

The study of epidemiology has enlarged their numbers, and now shows us that many diseases once regarded as constitutional or septic are in reality specific and pathogenic or mostly parasitic.

The prevention and limitation of this class of maladies must ever, therefore, largely occupy the attention of all of those who study the causes, the courses, and the results of disease.

The study of their etiology is always a fundamental inquiry. While to a degree it is possible to treat a disease skilfully without knowing its causes, it is always more satisfactory and generally more skilful to know something of the causes.

But what do we mean by etiology or causes? Surely not always, not generally, the beginning or efficient or final cause. Professor Semmola of Naples, at the recent meeting of the Medical Congress at Washington, may have startled some by these words: "Medicine, like all other sciences, never demands why, well knowing that the first causes of things are inaccessible, and that to every scientist it should suffice to know in which physical and chemical conditions this or that phenomenon manifests itself, so that he can modify and govern it at his will." This is true. The best that we can generally hope is to find the conditions, physical, chemical, or biological, in which phenomena manifest themselves. The word

'cause' is often in the same sentence used in two or three different senses. In our etiology we must remember that by 'cause' we mean mostly the 'conditions under which phenomena manifest themselves;' also that these conditions (thus called causes) mean the modifying influences present in the host or person, and the modifying influences of surroundings, much oftener than they mean any thing in the specific entity which we are so often calling the germ, and then calling it the cause.

While reaching back toward the beginning, even though we seldom reach the starting-point, we do come to see how it is true of every disease, as of every living thing, that it must have had a beginning. It is not a mere platitude to say that there was a day when the first case of small-pox occurred.

Nor does it necessarily belong to the sphere either of creation or spontaneous generation. A case of some new disease may put in an appearance, as did cholera on the Ganges, or a case of a known and existing disease may happen that is not derived from another precisely like it, but occurs because analogous conditions to those which gave rise to the first case of the kind have again occurred, and produced a pronounced deviation.

So it is possible that at the same time, as to some one disease, we may have cases of a communicable disease which have arisen from previous cases, and other cases that have resulted from a combination of influences or conditions such as gave rise to the first case. There are at least two reasons why the old dictum of *omne vivum ab ovo* cannot now be applied with the precision or with the finality with which it used to be quoted.

I. There is such a thing as evolution, which, while recognizing an original type, also recognizes departures from the normal which may have come to be so representative and paramount as to constitute newness in all essential particulars. Since we have come to recognize that many diseases are but developments and cultures of microphytic or microbic life, we very appropriately turn to the facts of botany, not only for illustration, but for verification of our theories. And what a change has taken place in its facts since the days of Linnæus! We no longer cling to the divisions of orders, genera, and species so closely laid down by him. We recognize two forces,—nature or heredity, and environment. A plant inherits a likeness which it tends to retain, but it is often so modified by environment as greatly to change, and so sometimes as even to lose, its identity. Environment comes to predominate over heredity. The horticulturist often takes a plant which he has found to be subject to variations, and fixes and perfects it in some one of them by cultivation.

Professor Huxley has recently contributed to the Linnæan Society a paper on the classification of gentians, in which he claims that gentians are all specialized; that is, become gentians from some other form. Permanency of type has so many exceptions, that variations of type, and the power to give fixity to some of these variations by means of cultivation or environment, must be accepted as a doctrine and a fact. Species and genera have variations, sports, modifications not dreamed of by the earlier botanists. Some of these departures are so marked and so predominant as to obscure the relationship and so far ignore it as to have individuality of their own. If, as we know, cultivation or surroundings can change a poisonous plant into a mild one, or can wholly modify it, it is not remarkable that a microphytic disease should lose its apparent identity, and at length in a new culture-medium, or under special conditions, become specialized. It is a law abundantly illustrated in the vegetable world, that environment causes variations, and that some of these variations tend to fixity of type, while others do not. All the wonderful facts of evolution show full well that we may in this way have what in respect of symptoms and treatment is a new disease. Yet it is not a *de novo* origin in an absolute sense, or, if practically *de novo*, it is not *de nihilo*. It is, that a

¹ Continued from *Science* of Dec. 9.

series of changes has been evolved by environment, by conditions in persons or in surroundings, until the result of some of these changes becomes self-assertive, and prevails over its heredity so as to secure for a longer or shorter period a fixity of its own. This fixity may be a new disease.

II. The history of hybrids, the so-called accidents of their occurrence, and the fertility of hybrids, are such that we are forced to look upon them as new forms of life — as becoming established into an autonomy or individuality of their own.

The horticulturist in some of the wonderful productions of new plants in the last score of years has come to be familiar with hybridisms that wholly obscure origin, and that come to have a fixity of their own as much as some new diseases with which we have to deal, or as much as cases of disease which we cannot trace to a contagious source.

All this has been greatly emphasized by what we have come to know as the fertility of hybrids. Not over thirty years ago the general view of botanists was that hybrids were sterile. But so numerous are the exceptions, and so abundant have been the results of new cultivations, that this view is fully disproven. Says a recent botanical authority, "Among plants there are many instances of hybrids between species being perfectly fertile, and continuing so for an indefinite period. Experiments during the last twenty-five years have increased the number of such fertile crosses manifold. . . . Fertility of hybrid plants is the rule, and sterility the exception. So far as plants are concerned, there is not the slightest ground for considering sterility as a distinctive bar, separating species. These hybrids come to have a specificity of their own so different from the parentage as to be unrecognizable, and so specialized as to be permanent." As another expresses it, "The hybrid becomes an individual not responsible to its species."

Nor is this confined to plants with spores. Some of our most skilful horticulturists are now producing varieties of ferns by hybridism, and every now and then some so-called chance growth or sport shows wide departures.

The bearing of all this on the parasitic forms of disease is not far to seek. If, as now seems so nearly proven, so much of disease has to do with elementary and minute forms of vegetative life, it is easy to see how the facts of evolution and hybridism have a bearing upon the appearance and propagation of disease. The light of the botanical world and its marvellous revelations as to the actualities and possibilities of the origination of new forms distinct from the parentage, penetrates the hidden sphere of disease-origin, and shows how some diseases cease, how others arise, how some lapse back to their heredity, while others are made permanent or specialized by their environment, and how others still are hybridized into specific forms, and acquire a fertility and fixity of their own. This range of deviation is so wide as to account for very many of the anomalies of disease, and for the organization, cessation, or modification of form.

In reply to my inquiry on this particular point, Mr. Mehan, the distinguished botanist of Germantown, Penn., writes me, "All hybrids, that we know by actual experiment *are* hybrids, are fertile as far as I know, and reproduce their originals just the same as if they were 'original' species. I think almost every botanist of note believes in abundant, fertile hybrids in nature. Coming down nearer to your own line of thought, I believe it is conceded that all lichens are hybrids between fungi and algæ. It is tenable that new forms of disease are continually coming into existence, which can only arise from new forms of disease-producing plants (microscopic fungi) being evolved from older species; but I do not know whether there is an opinion that these new forms are the result of hybridization or of that natural law of change which seems to be a constituent part of existence — in vegetation, at least."

In a paper read by Dr. M. W. Taylor before the Epidemiological Society of London, April 13, 1887, he notes the fact that many of the fungoid varieties were but "conversions of elementary states of penicillium and oidium. It is also maintained by Zopf (the botanist) that there may be a pleomorphism amid pathogenic micro-organisms, and that there are stages of intermediate forms resulting from the nature of the nutrient media" (see *Lancet*, May 7, 1887, pp. 933, 934).

The view is, we believe, gaining ground, that harmless micro-

phytes may become pathogenic, and that the different forms of micro-organisms present have relation to each other, and that the culture-medium in disease, viz., the person and his surroundings, determines the character of the micro-organisms full as much as the micro-organisms determine the character of the disease.

If the views as to the microphytic origin of most of the communicable diseases are correct, the study of the laws of this evolution and hybridism is vital. We believe it is in this direction that we are to account for the origin of new diseases, or for such variations in type as obscure or destroy identity. If we can, through this study, arrive at the evidence that in this sense many diseases begin, we have a new department of study, in that we are called upon to define with accuracy how and why this origin takes place, in order that we may thwart or circumvent the conditions.

If it is the result of evolution through a long series of changes, it behooves us to study the normal, and to watch and record all the gradations by which the unfriendly result is attained, so that, at some stage, we may intercept the progressive and threatening changes that are occurring, or ascertain what condition of the person, or what condition of surroundings, constitutes the influence which brings about the change, or provides the fertilizing medium for the disease, and causes it to break forth. If it is the result of a hybridism which occurs spontaneously or rapidly, we need to study precisely what forms of vegetative life thus incline to coalesce, under what condition the union occurs, and how their conjunction, development, and fertility are to be interrupted.

If special conditions of some parts, — as the throat, for instance, — or certain conditions of the secretions, furnish a special soil or culture-fluid for the propagation of low forms of vegetative life, this is to be studied with exactness.

In each of these lines the same method of technical study and close record and analysis of facts by competent observers which has prevailed in the study of minute plant-life by botanists, and which has obtained in many other sciences, will, in this comparatively new field of biological and botanical research, accomplish equally valuable results. In it we are attempting to find out how much and under what circumstances micro-organisms imperil human life.

The practical value of such an inquiry is apparent, for sooner than is the case with most of the studies of nature, the results will be applicable in the prevention and treatment of disease. It will be a great gain if we can come to know, that either under the laws of evolution, or as the result of admixture or hybridism, symptoms and pathological effects become specialized so as to constitute a new disease which maintains its type.

As examples of how proximity of different diseases may modify symptoms, we have many suggestive facts in the history of disease. Yellow-fever is believed by many to be a mongrel, born on the high seas by admixture of the jungle-fever of Africa with the typhus of the pent-up hold of the filthy vessel. It is not certain that typhoid-fever was not once nearer to typhus, until it came to be called abdominal typhus, and then to have modifications because surroundings and the acquired power of self-propagation gave it an autonomy of its own.

It is not even now certain that there are not grades of cesspool and other adynamic fevers that will some day declare another well-marked departure from what we now call typhoid, and come to have an individuality of their own. It is not certain, that, when Sydenham treated scarlet-fever and measles as one disease, their lines of difference were as well marked as now.

Diphtheria so often seems to have a localized origin, and common forms of sore throat are so often seen to pass away from their general into a special type, that it will not be strange if we can come to the law of departure (see views expressed in the *Sanitary Record* of Aug. 15, 1887, p. 88, and the contribution of Dr. Wordin, in the *Connecticut State Health Report for 1887*, that diphtheria results from the special virulence of a micrococcus which is not specific, put present in forms of foul or septic sore throat).

While typho-malarial fever has no pathognomonic lesion to distinguish it from the ordinary typhoid, yet we do know it has symptoms to distinguish it. The advances of biological investigation have put us in regions of new possibilities, that do not involve spontaneous generation, but yet do render probable what is equiva-

lent to the *de novo* origin of cases of disease which afterward are chiefly communicated by the first and succeeding cases.

Having settled that such origins do take place, we shall then pry into the secret of the laws of combinations and the conditions which favor the evil evolution or the facts of hybridism, and seek to combat these by starting similar processes in opposite directions, or by sterilization, neutralization, disinfection, and all the details of radical sanitation.

Such a view of the occurrence of old or new diseases, and of the reasons for fixity in some and changing forms and types in others, leads to several practical results.

1. The study of parasites, or germs, as they are called, is only one of the methods of informing ourselves as to the phenomena of disease, and in itself is not likely to be the key to rational and successful treatment.

2. Our attention should be directed, far more than now, to the study of conditions and circumstances under which new forms appear; to the influence of persons and surroundings, instead of to the mere finding of a specific form. The latter would, of course, be most valuable as one of the facts in the chain of evidence, but we should not, as now, seek so much to look to it as the cause of disease as to inquire what conditions have caused this or that particular microphyte to be present.

3. We should be able to account for the occasional occurrence of a disease independent of any previous case, and for changing types of disease and new diseases, and would come to treat diseases less by their names and more in view of their type and the effect of surroundings upon them.

4. The tendency of all this is to magnify the importance of close observation, and to lead us to feel that success in warding off disease, and in treating it when it appears, depends mostly upon close observation and that experience which is derived from actual practice.

If we are looking to the biological laboratory for the natural history of disease, or to the chemical laboratory for the application of remedies, we shall surely fail. It is not so much that we need to find the specific germ or the specific methods. The world is always looking after specifics. But the science and art of sanitation has far more to expect from a study of the conditions of persons and surroundings under which diseases, or types and modifications of disease, manifest themselves, as also from a study of the prevention or obliteration of such conditions, than it has to expect from the finding of microphytes as the source of disease, and seeking to cure disease by expelling micro-organisms or attenuating them.

Our only design in this paper is to awaken inquiry as to modes of accounting for the localized origin of disease, without any antecedent case, on the proposition that the laws of evolution, environment, hybridism, or modification by culture, give rise to diseases so different from their prototypes as to have individuality and permanency of their own.

Because such inquiry is relevant to prevention, there is good reason to believe that by ascertaining the laws of these transformations and modifications of type and of the origination of special varieties, we shall ere long find new means for the prevention or limitation of many diseases.

'The Malarial Germ of Laveran' was the title of a paper read by Dr. William T. Councilman of Baltimore. He considers that this organism probably belongs to the *Protozoa*, a group of unicellular organisms noted for the varied changes of form which the individual examples undergo in the course of development. Of the malarial germ there are ten more or less distinct forms, of which five are always found in intermittent-fever. During the chill of the fever a definite form is seen, in which multiplication takes place by segmentation. One form has actively moving filaments. This was found in blood taken from the spleen in ten cases of malarial cachexia, and in five cases of intermittent-fever. Dr. Councilman says that too much importance cannot be assigned to this organism as a means of making a differential diagnosis between malarial-fever and typhoid-fever. In outbreaks of fever which occur in small country-towns, where it is of the greatest importance that the character of the disease should be recognized promptly, the advantage of this mode of diagnosis is most evident. There is too much

reason for believing that in localities where malarial-fevers prevail, epidemics of typhoid-fever are frequently mistaken for fevers of a malarial type.

Mr. H. Lomb of Rochester offered prizes of \$500 and \$200 for the best essays on practical sanitary and economic cooking adapted for persons of moderate and small means. Dr. LaBerge, health-officer of Montreal, described the system employed in that city for the collection of garbage, and for its destruction by the Mann furnace. Committees were appointed on State boards of health, pollution of water-supply, disposal of garbage, animal diseases and animal food, forms of statistics, incorporation, protective inoculation, Lomb prize essays, national health legislation, and improvement of the sanitary and medical service on emigrant ships. It was decided to hold the next meeting of the association at Milwaukee. The following officers were elected for the ensuing year: president, Dr. Charles N. Hewitt, Red Wing, Minn.; vice-presidents, Drs. G. B. Thornton, Memphis, and Joseph Holt, New Orleans; executive committee, Drs. Henry B. Baker, Michigan, S. H. Durgin, Massachusetts, and J. N. McCormack, Kentucky. The secretary, Irving A. Watson, M.D., of Concord, N.H., holds over.

THE SURFACE-TEMPERATURES OF THE OCEANS.

A NUMBER of researches on the surface-temperatures of the oceans, which have recently been published, throw a new light on this complicated phenomenon. The maps accompanying the present number of *Science* have been constructed according to Dr. O. Krümmel's maps, showing the surface-temperatures of the oceans. As the Arctic Ocean must be considered part of the Atlantic, of which it forms the most northern extremity, it was desirable to include it in the map. Besides this, the Antarctic Ocean exerts a great influence upon the southern part of the Atlantic Ocean, and therefore the latter has also been included in the map, which shows two-thirds of the earth's surface in a perspective projection. The lateral parts, however, have been left off, as they do not belong to the system of the Atlantic Ocean. The Pacific Ocean has been constructed in the same way, the map extending from its northern limits to the entrance of the Atlantic Ocean. The latter map makes it very clear that the Pacific Ocean forms a comparatively well-defined basin connected by narrow straits with the basin of the Atlantic and Indian Oceans. Its southern limit is indicated by the east coast of Australia, Wilkes Land, Graham Land, and the southern portion of America.

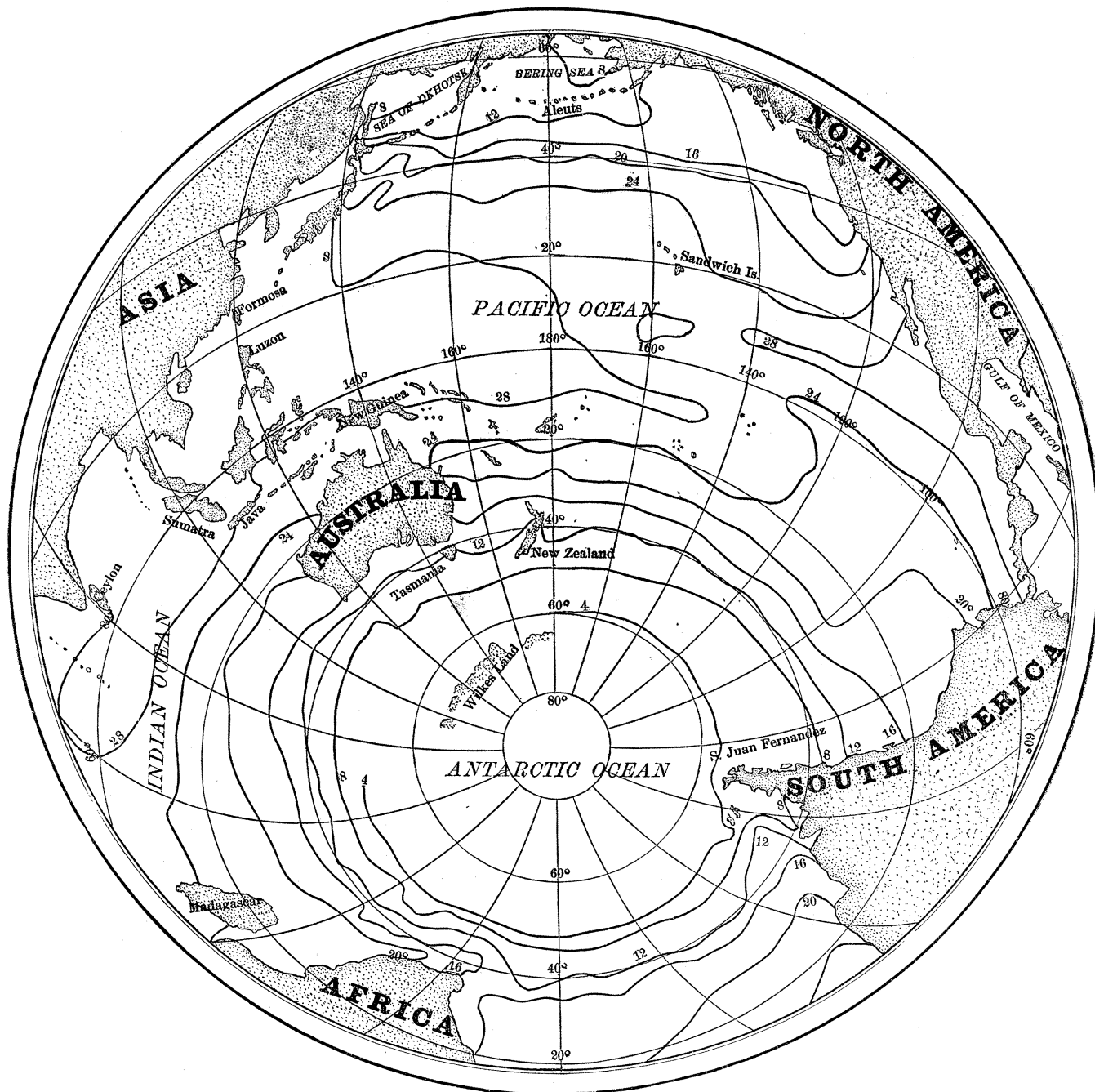
A glance at the lines showing the surface-temperatures of the oceans reveals the remarkable fact that the warm water is accumulated in the western parts of the oceans. Krümmel designates water of more than 24° C. (75° F.) as 'tropical water.' In August the belt of such water is 21 degrees of latitude wide in the eastern part of the Atlantic, while it occupies 61 on its western side. In February it is 22 degrees wide in the eastern part, while it is 56 degrees wide in the western. In the Pacific Ocean it does not occupy more than 17 degrees in August and 25 degrees in February, while in the western parts its width is 57 degrees and 49 degrees respectively. In comparing the amount of tropical and extratropical water, Krümmel finds that twenty-nine per cent of the whole surface of the oceans has always a temperature of more than 25° C., while almost one-half of it temporarily attains this temperature.

It will be observed that in certain parts of the oceans the lines of equal temperature are much crowded, and show sharp angles. This is entirely due to currents, which carry warm water to high latitudes, and cold water to warmer regions. Thus the influence of the Agulhas current may be observed in the sharp angles of these lines near the Cape of Good Hope, while the cold Cape Horn current lowers the temperature along the eastern coast of South America. The influence of the Gulf Stream may be seen in the crowding of the lines of equal temperature near Newfoundland.

The accumulation of warm water in the western parts of the oceans is entirely due to the action of the trade-winds, which blow continually from the eastward, and drive the warm water of the ocean westward, where it is accumulated on the coasts of the continent. Buchanan has explained this phenomenon in a paper on similarities in the physical geography of the great oceans, which has been published in the *Proceedings of the Royal Geographical*

Society. The trade-winds, he says, produce not only dry places on the land, but also comparatively dry places on the sea, and therefore the areas of maximum density or salinity of the surface-water are situated in the trade-winds region. In passing over the surface of the ocean, the winds impart motion to the water immediately under their influence. The effect of this is to produce a general motion of the denser intertropical water towards the equa-

west coast of North Africa the line of 24° C. (75° F.) extends far southward. Similar phenomena occur on the west coast of America. Formerly the reason for the low temperature of these waters was looked for in polar currents, but recent observations show that it is caused by cold water of deeper layers rising to the surface. So far as we are aware, E. Witte was the first to express this opinion, in a paper published in 1878; but recently a considerable



MAP OF THE PACIFIC OCEAN, SHOWING THE SURFACE-TEMPERATURES IN AUGUST (ACCORDING TO O. KRÜMMEL).

tor and towards the west. At the same time the surface-water is evaporated, and thus becomes more concentrated than the deeper and colder layers. In consequence of its greater salinity, the warm surface-water sinks to a certain depth, and thus conveys its higher temperature to the deeper layers. Thus the western parts of the ocean are supplied with water of high temperature, which is collected in the Atlantic Ocean in the immense bay formed by the coasts of North and South America.

Another remarkable fact is shown in our map. It is the prevalence of cold water along many coasts. In South Africa we see the line of 16° C. (61° F.) extending far northward, and on the

amount of material has been contributed by numerous investigators. Among these, I mention Buchanan's researches in the region of the counter equatorial current on board the 'Buccaneer,' Captain Hoffmann's observations off Cape Guardafui, and Dr. G. Stapff's off Angra Pequena in South Africa. All these observations tend to show, that wherever the prevailing winds are blowing off the shore, and the water is thus removed without a possibility of being replaced by superficial currents, cold water rises to the surface. But it has been pointed out in an essay published in the *Annalen der Hydrographie*, that, wherever a current is deflected from a coast, cold water must rise to the surface. This fact accounts for the low

temperature off the west coasts of California and South America. A number of profiles showing the temperatures of the Pacific Ocean off the coast of California, which were published by Dr. C. M. Richter in the *Bulletin of the California Academy of Sciences*, show very plainly the rising of cold water near the shore; and although the author tries to prove, by means of these charts and profiles, the existence of a cold current, they seem to be far more in favor of the theory advanced above.

It will be seen that in the equatorial parts of the Atlantic Ocean two regions of remarkably cold water occur. One of these is on the coast of Guinea; the other, east of St. Paul. Krümmel believes that they are also due to a submarine source, the cold water of the depth taking the place of the warm dense water which is driven westward by the wind. He points out that it is situated between the equatorial current and the counter equatorial current, and that thus the cold water supplies a deficiency caused by two currents flowing in opposite directions. Therefore this area of cold water does not exist in February, when both currents are less strong. The Guinea current he considers entirely caused by the southern equatorial current, and as supplying the Gulf of Guinea with water instead of that which is drawn from it by the southern equatorial current. We ought to point out here the fact shown by Buchanan, that all counter equatorial currents are very superficial, that their velocity is the greater the less the density of this water, and that the isothermal gradients are very great below these currents, as the light water of the surface prevents the heat from penetrating into the ocean.

The problems of the equatorial circulation of the oceans is extremely complicated, and the observations mentioned above show that the vertical as well as the horizontal circulation of the waters must be studied. The dynamics of the counter equatorial current are particularly obscure, and a careful investigation of its density, temperature, and strength is very desirable.

MENTAL SCIENCE.

The Mechanism of Attention.¹

VOLUNTARY attention is an artificial act: it grafts itself upon spontaneous attention, and takes its nourishment from this. In spontaneous attention the object acts by its intrinsic power; in voluntary attention the subject brings an alien power to bear upon the process. Spontaneous attention represents the maximum of attraction between subject and object; voluntary attention, the maximum of resistance. It is the voluntary form of attention that is here to be considered.

In the first place, how is so artificial a process as voluntary attention brought about? The method, says M. Ribot, is to make attractive artificially what is not so naturally; to arouse an artificial interest in things naturally uninteresting. The process by which this is done is infinitely varied, but consists always of arousing an interest by playing upon some emotional state.

The infant, according to Preyer, at first is under the sway of spontaneous attention alone: it looks only at bright objects, at sustenance-giving objects. At about the end of the third month it explores the field of vision, and glances at less and less (selfishly) interesting objects; and it is the same with the other senses. The path is from the most intense, most impressive sensations to the finer, more delicate ones. The child naturally flits from one sensation to another: to fixate and hold one sensation is an art that must be learned. A child, for example, refuses to learn to read, but is vastly interested in the pictures in the book. The father says that reading will show the meaning of the pictures. This acts as an artificial inducement, and the child goes to work, substituting an artificial attention to arbitrary signs for the natural attractiveness of pictures. M. Ribot distinguishes three periods in this substitution process. In the first we can appeal to bodily feelings alone. The child can be taught voluntary attention only by playing upon its fear, its egoistic tendencies, by rewards, sympathetic emotions, natural curiosity. In the second period the emotional nature is still the most powerful motive, but the kind of emotion is higher. One can here appeal to ambition, to emulation, to duty. In the final period the attention is maintained by habit. The student at his

desk, the workman at his shop, often wish they were elsewhere; but the habit as formed by past appeals to pride, ambition, etc., chains them to their tasks. Art has done its work, and attention has become second nature. Granted a certain environment, and the work goes on almost of itself. Many persons never reach this third organized stage of voluntary attention: there is a vast body of unsteady, Bohemian, vagabond types of character in whom voluntary attention is sporadic only, and not habitual.

The training of animals proceeds by the same steps. An ape is taught to do things meaningless to it by connecting such acts with rewards and punishments. The factor of attention in the process is well shown, in that such animals are selected for training as most readily attend amid distractions.

The genesis of voluntary attention is to be found in its utility. When the conditions of life become at all hard, and especially if they become so by more or less sudden changes, the power of adaption to them is dependent upon voluntary attention to details; upon consideration of something besides the immediately attractive and useful. The savage is lazy, is inspired only by chase, by war, by play; his interest is in the unknown, the unforeseen, the chance. He is not capable of continuous labor. In half-civilized communities work is repugnant. Voluntary attention is a factor of civilization, and is maintained with effort. The most constant characteristic of criminals is lack of power to pursue a steady calling; and the Italian anthropologists regard this as a reversion to primitive habits. Voluntary attention thus came in, and is maintained as a sociological power.

While we all have quite a definite notion of the feeling of effort in fixing the attention, the nature of the process escapes our observation. We feel that the struggle is to focus the thought upon one topic to the exclusion of others, all knocking for admittance into consciousness. The question is not, 'How does a concept come to be attended to?' but 'How is it maintained in the focus of attention? How do we inhibit other concepts?' The answer is very incomplete. The physiology of inhibition is in its infancy. The fact itself simply states that the excitation of a nerve may not only produce motion, but may cause a motion to cease. Stimulating the pneumogastric nerve arrests the heart-beat. The highest form of this power of inhibition is attention: this Ferrier locates in the frontal lobes of the brain. The intelligence would thus be proportionate to the development of these lobes; stimulation of them would never produce movements; and their disease would cause no paralysis, but a lowering of the mental life. All this is found to be true. Inhibition is likewise late in appearing, coming long after impulsive will in child-development. We know only the initial and final steps of the process, the will not to do an action, the fact that it is not done; but we have good reason to believe that the muscles play an active rôle in the process.

Attention may be fixed upon three kinds of mental objects, — perceptions, images, ideas. In perception the dominance of the motor element is evident. In seeing, touching, etc., there is always a motion; and the law that the more mobile the part the more sensitive it is, is quite a general one. To fixate an object steadily without moving the eye soon reduces the field of vision to a blank; a weight constantly pressing upon the skin is soon not felt. Consciousness is always of change, and change is based upon movement. Attention is the repression of foreign, irrelevant movements and the concentration upon pertinent ones. Distraction is a diffusion of movements. Next with regard to images. Here the attention is turned inwards, and becomes reflection; but the motor element has not been lost. The motor element of the perception is only weakened (not lost) in its recollection. The two processes are the same in kind, and differ only in degree. As the vividness of the recollection increases, it approaches the perception in the prominence of the motor element. The intense thought of falling down an abyss has led some persons to throw themselves down. Mind-reading is really muscle-reading. When we pass to ideas (and especially abstract ideas), the problem is more difficult. M. Ribot confines his attention to three types of ideas. The first are such as are formed by the fusion of like images without the aid of a word. Their type is the idea of a class, a species. This is within the grasp of animals, children, and deaf-mutes. It is a generic image, like a composite portrait. Here the question as to the motor

¹ A résumé of an article by Prof. Th. Ribot (*v. Science*, No. 252).

element is unnecessary, because there is no voluntary reflection involved in attaining this idea. The second class includes the fusion of images in different objects, still without the aid of words, and is represented in the common abstraction. This is a much higher process, and many peoples have stood still at the early stages of it. The Fuegians have no abstract terms; the Indians have words for 'red oak,' for 'white oak,' but not for 'oak' in general; the Tasmanians have words for different kinds of trees, but not for 'tree.' In all these processes there is a motor element in the word, and perhaps in the image too. Finally, as the image becomes more and more abstract, the word becomes more and more fundamental. That the word contains a prominent motor element (varying, however, in its strength in different persons) is generally accepted, and is shown by the fact that this element in language can be lost while the rest remains unlost.

We are thus led to the conclusion that thought without motion is impossible; and, though we cannot have the opportunity of demonstrating this absolutely, we can make it extremely probable. Severe activity is incompatible with intense thought. To direct one's attention is work, and the less natural interest in the topic the more fatiguing the strain. It is not a purely mental process, but is connected with nerve-activity and such movements. Monism is work, destruction is rest.

Finally the hints as to the action of attention to be derived from experimental study should be noted. In re-acting to a stimulus, the time is shortened when the attention is fixed, and is lengthened when the attention is wandering. So, too, the more cultured classes can re-act more quickly than the ignorant, because their power of voluntary attention is drilled. If the physical state prevents sharp attention, the time is lengthened. A headache lengthened a re-action-time from .133 to .171 of a second, and severe fatigue to .183 of a second. In various stages of paralysis the time lengthened to .166, .281, and .755 of a second; while in hypnotism, when there is an extraordinary concentration on one perception, the time was shortened from .328 to .193 of a second. Again: the most influential factor in the re-action-time is the expectedness of the impression. If the sensation is preceded by a signal announcing its approach, the time is much shortened. According as the nature, intensity, and time of the stimulus is known, the time is more and more shortened. The unexpected delays the re-action. Again: Wundt has shown that when two impressions—say, the ring of a bell and the movement of an indicator—are simultaneous, the one that is attended to gets first perceived. The adjustment or accommodation of the attention in all these cases is again a motor act.

SCARLET-FEVER REPORT.—I.

THE success which has followed from the collective investigation into the subject of distillery-milk and its effects on the lower animals and man when used as food, which was made by *Science* in June, has induced this journal to undertake other inquiries into similar matters which affect the public health. Correspondence has been opened with a number of prominent sanitarians of the United States, and as a result scarlet-fever has been selected as the next subject for inquiry. The following letter has been prepared, and forwarded to leading sanitarians and physicians, and others:—

The prevalence of scarlet-fever in all parts of the civilized world, and the great mortality therefrom, amounting in England alone during five years to 88,273 deaths, have induced *Science* to institute an inquiry into the reasons for such a condition of things,—whether it is a fact that this disease is not amenable to control by sanitation; or whether sanitarians have not suggested any practical method by which it may be controlled; or whether parents, teachers, health authorities, and others neglect to carry out the recommendations which sanitary science has made. With the object of helping to determine these questions, will you kindly answer the following inquiries:

1. Do you believe that scarlet-fever ever arises, at the present time, *de novo*, as distinct from a pre-existing case? If so, on what grounds do you base that belief?

2. Is there any doubt in your mind that scarlet-fever is a com-

municable disease, and, if so, what reasons have you for that doubt?

3. If you believe it to be communicable, can you give any instances which have come under your own *personal* observation, tending to prove its communicability? If so, please give them in detail.

4. Have you any information touching the communication of bovine scarlet-fever to man, either by contagion or the milk of the affected animal?

5. When does a patient who has had scarlet-fever cease to communicate it to others?

6. Can you give any instances which have come under your own *personal* observation in which clothing, toys, books, or other articles have communicated the disease? If so, please give them in detail.

7. How long have you *personally* known such articles to retain the infection?

8. Should boards of health require reports of cases of scarlet-fever to be made to them, and, if so, by whom and why?

9. What is the duty of boards of health if such reports are received?

10. Is there any plan which, if put into execution, would, in your judgment, prevent the spread of scarlet-fever?

11. If so, can you give instances in which it has practically done so?

12. Do you believe that any thing can be done, by the use of remedies or otherwise, to prevent well persons from contracting scarlet-fever when they are exposed to it?

13. Can you give any *evidence* not under your own personal observation, but sufficiently authenticated by competent authorities, printed or otherwise, touching any of the questions propounded in this circular?

To these inquiries a large number of answers have been received, which we now propose to lay before our readers:—

[WILLIAM K. NEWTON, M.D., Paterson, N.J., State dairy commissioner.]

Below please find answers to your circular relating to scarlet-fever:

1. I have often seen isolated cases of this disease beginning at a time when no other case existed in the city. Many times I have seen a single case begin without any probability of an exposure to another case, but I do not think that we are justified in accepting the theory that the disease may arise *de novo* because of our inability to find the original case. But there is much to lead us to study this side of the question, for filth may be a possible cause.

2. It is no doubt communicable.

3. It is communicable, and scores of instances might be mentioned to substantiate this statement. Cases where children have been exposed at school for a few minutes to one sick with the disease have come under my notice, and, where they have not had the disease before, they have taken the disease in due time. It is a common occurrence for children exposed to the sick to contract this disease.

4. No personal information. The London *Lancet* during the past two years has contained many articles on this subject.

5. As long as there is any roughness of the skin.

6. Have known of many instances where woollen clothing has been the means of carrying the disease.

7. Three weeks.

8. Yes, if the proper officers are prepared and authorized to enforce strict separation and quarantine. If reports are required, and on receipt by the authorities are filed, or only tabulated, and no repressive measures employed, no good is done; and the reports should not be required, for they only go to swell the statistics without benefit. The attending physician is the proper person to notify the authorities, and for this work he should be compensated. Reports from the householder or the family are not reliable, and are not promptly made. As was said, these reports are for the purpose of enabling the health authorities to restrict the spread of the disease, and, if notification is required, restrictive measures should be followed up.

9. The reports should first be recorded, and the location of the case marked on a map of the city kept for that purpose, the latter entry being a record of location to enable us to ascertain if locality

has any thing to do with the disease. Next the proper officer should be sent to the house to instruct the family in methods of disinfection, and to note the condition of the house. He should leave with the family a circular, issued by the board of health, giving an account of the disease, its methods of spreading, and its dangers, also brief directions for disinfection. On the recovery of the patient, or in case of death, the premises should be disinfected by an officer of the board. People from infected houses must be excluded from school, church, factories, etc., and a strict quarantine maintained until the premises shall have been disinfected. In case of death, the body should be interred as speedily as possible and no public funeral allowed. No person should be allowed to enter the house except the regular attendants.

10. If the above plan is rigidly carried out, it is possible to check the spread of the disease; but at present it is not possible to strictly quarantine or isolate cases of this disease, because many think it a mild one and of little consequence. Again, many cases are so mild that the sick person is not confined to the bed or even the house, and hence mingle with the well, and these mild cases are often not recognized.

11. I know of no instance where the spread of scarlet-fever has been certainly and completely stopped by restrictive measures; but could we enforce rules as rigidly as is done in epidemics of small-pox or cholera, where the public is prepared for harsh methods, it is probable that the disease might be stamped out for a time.

12. I know of none.

[SAMUEL W. ABBOTT, M.D., Boston, Mass., secretary State Board of Health of Massachusetts.]

In answer to your circular relative to scarlet-fever, I have the honor to reply as follows; my reply being based upon an active country-practice of twenty years, and also upon five years' experience as the health-officer, and secretary of this board, during which time I have had opportunity to observe a considerable number of epidemics of scarlet-fever of varying extent and severity.

1 and 2. No.

3. An instance under my observation is the following: A health-officer visited a public institution for the purpose of giving advice as to preventive measures. While there he saw several children ill with scarlet-fever, in various stages of the disease, and examined them at the bedside. He then returned to his home, a hundred miles distant. On his way home, and before returning to his family, he took the precaution to take a thorough bath, and, on reaching home, sent his clothing to a cleansing establishment. About eight days afterward, three of his children were taken ill with scarlet-fever, one of whom died on the fifth day afterward. There were no other cases of scarlet-fever in the town at that time.

4. A limited outbreak occurred last spring in the town of Wellesley. Four children were taken ill in two families who took milk from one man who had two cows. There were no other cases in the town at that time. One of these cows had an eruption on the udder, which, on examination by a competent veterinary surgeon, did not appear to have the characteristics described by recent English reports. I do not regard this as a conclusive case.

5. Not until after desquamation has been completed, and all germs of the disease originating from the patient have been destroyed.

6. I have no doubt that such is the case, but have no positive evidence on this point.

8. Yes, by the attending physician, who is the only competent authority to judge of the character of the disease. The law of this State makes it incumbent on both physician and householder to report, but reports from the latter are rare.

9. To isolate the sick, and see that proper measures are taken for the continuance of such isolation, and of disinfection upon recovery; and in case of death the funeral should be private.

10. Nothing short of complete isolation of the sick, and absolute destruction of all germs of the disease.

11. The following statistics are suggestive. By circulars, reports of boards of health, public lectures, and by every possible means, the contagiousness of scarlet-fever has been taught, and the community has been led to see the dangerous character of the disease, and its destructiveness as compared with other diseases, until public opinion will tolerate much more decisive measures than were possible twenty years since. The following table (from the

'Massachusetts Registration Report for 1886') shows that the disease has not been so destructive in the past fifteen years (1872-86) as it was in the previous fifteen years (1857-71) in this State:—

Mortality from Scarlet-Fever, 1857-86.

YEARS.	Deaths.	Percentage of Deaths to Deaths from All Causes.	Death-Rates per 10- 000 Living.	YEARS.	Deaths.	Percentage of Deaths to Deaths from All Causes.	Death-Rates per 10- 000 Living.
1857 . .	2,013	9.36	17.2	1872 . .	1,377	3.93	8.9
1858 . .	1,051	4.99	8.8	1873 . .	1,472	4.34	9.4
1859 . .	1,038	4.88	8.6	1874 . .	1,382	4.33	8.6
1860 . .	916	3.92	7.4	1875 . .	1,684	4.81	10.2
1861 . .	1,137	4.66	9.2	1876 . .	1,222	3.79	7.4
1862 . .	1,261	5.42	10.1	1877 . .	467	1.53	2.8
1863 . .	1,399	5.01	11.1	1878 . .	404	1.33	2.4
1864 . .	1,503	5.21	11.9	1879 . .	850	2.63	4.9
1865 . .	807	3.06	6.4	1880 . .	574	1.63	.2
1866 . .	385	1.58	2.9	1881 . .	397	1.09	2.2
1867 . .	828	3.63	6.2	1882 . .	318	.87	1.7
1868 . .	1,369	5.35	9.1	1883 . .	575	1.52	3.5
1869 . .	1,405	5.39	9.9	1884 . .	627	1.69	3.3
1870 . .	683	2.49	4.7	1885 . .	587	1.54	3.0
1871 . .	867	3.10	5.8	1886 . .	331	.89	1.7
Average 15 yrs. (1857-71)		4.51	8.5	Average 15 yrs. (1872-86)		1.97	3.9

Undoubtedly several extensive epidemics might upset these statistics. I think, however, that the length of time included (30 years), and the size of the population (1,231,067 in 1860, and 1,942,141 in 1885), may be taken as conclusive that something has modified the progress of the disease, the death-rate in the last fifteen years being less than one-half as large as that of the fifteen years previous (1857-71). If the period of ten years 1857-66 be compared with the last ten years (1877-86), the result is still more striking, the death-rate of the latter period from scarlet-fever being less than one-third as great as that of the former period.

12. By remedies. No.

13. I beg leave to mention one or two popular errors in this connection. There is a singular notion that physicians' families are comparatively exempt from infectious diseases. On the contrary, I can call to mind at least a dozen families of country physicians in active practice, within five miles of my present home, which have been invaded by this destructive disease. Another error is the notion that a dead body is more dangerous than a living one. There is no evidence, so far as I can learn, in support of such a notion. One living, breathing body, sick with infectious disease, must necessarily produce more infectious material than a dead one.

[R. B. S. HARGIS, M.D., Pensacola, Fla.]

In relation to the questions concerning scarlet-fever stated in your circular, I proceed at once to answer them in order:—

1. I have seen scarlet-fever on two occasions, in an isolated house,—no other cases in the city nor in the neighboring places,—the source of which could not be traced to any focus of infection outside the walls of the dwellings in which the cases existed. I have also seen measles, a congener of scarlet-fever, undoubtedly a contagious disease, spring up spontaneously, and spread from a single isolated place all over the city, thus apparently arising *de novo*. Upon careful and anxious inquiry in regard to the above-mentioned instances of spontaneous development of scarlet-fever and measles in isolated places, I was informed that none of the parties in those houses had had any correspondence, by letter or otherwise, or purchased or received any fabrics, or indeed articles of any kind, from persons at a distance anywhere. All infectious and contagious diseases are associated, as a rule, with cutaneous eruptions or glandular

affections, and capable of propagation by inoculation. Whooping-cough may be an exception so far as eruptions and glandular affections are concerned.

2. I have not the least doubt of the communicability of scarlet-fever. I know that it can be propagated by *fomites*. Diseases that can be communicated and transmitted by *fomites* are absolutely contagious: this is self-evident, and cannot be disputed with any show of reason.

3. I know a remarkable instance of the communicability of scarlet-fever by things. It occurred in my own family, living more than two thousand yards from any infected place. The subjects of it got the disease from the clothes of a person who had visited a house in which there were two or more cases.

4. I have no information touching bovine virus.

5. I have known children who had had scarlet-fever, a week after the process of desquamation had been completed, and after antiseptic baths, and their clothes and house environments had been subjected to a disinfecting process, to return to school, and mingle freely with other pupils without communicating the disease to any one. Some writers in the medical journals express the opinion that from four to six weeks should be allowed to elapse before those who have recently had scarlet-fever be permitted to have free intercourse.

6. This question is answered in Paragraphs 1 and 3.

7. I have no experience with regard to the length of time the infection of scarlet-fever may be retained in any articles, but authors will be referred to when Question 14 is under consideration.

8. Boards of health should be empowered by legislative enactments in every State to enforce physicians, and the laity also, — every citizen who may know of a case, or even a 'suspicious case,' anywhere in his vicinity, or indeed in any place from which the disease might be introduced, — to report without delay to the proper authorities.

9. The duty of boards of health and others in authority is to act promptly on receipt of the report of the first case of scarlet-fever, and enforce isolation and disinfection rigorously; and it should be made incumbent on the attending physician to change his clothes, and bathe, etc., before visiting other houses not infected. I have known the morbid principle, both of small-pox and yellow-fever, to be carried in men's beards, and communicated to their wives and children. Indeed, the *materies morbi*, or germs of all contagious diseases, can be transmitted from place to place by any thing capable of absorbing, or holding adherent to it, any substance, dead or living, whether in a solid, fluid, or gaseous form.

10. Besides what I have already indicated in answer to Question 9, I know of no other plan of preventing the spread of scarlet-fever except inoculation and the administration of small doses of belladonna, which should be considered as only a small part of the plan already indicated.

12. As just stated in answer to Question 9, I should state that I gave belladonna to three children for whooping-cough, living in the same house, situated in the very heart of a district infected with scarlet-fever, which was epidemic, and whose parents communicated freely with persons who had the disease, and every one of them escaped.

13. Touching the questions stated in your circular, I can give no further evidence of importance as the result of my own personal observations, but refer to the following authorities, which I have selected from many others in my library: viz., Watson's 'Practice of Medicine' (American edition, edited by Dr. Francis Condie), p. 1180; Ziemssen's 'Cyclopædia of the Practice of Medicine,' vol. ii. pp. 161 *et seq.*; Aitken's 'Science and Practice of Medicine,' 6th edition, vol. i. p. 480.

Scarlet-fever, in my opinion, is certainly amenable to control by proper sanitary measures under ordinary circumstances; but if a case occurs in a town or city where there exists a certain condition of the atmosphere, or certain meteorological states of the air (of which we know very little, and which has been significantly termed 'epidemic constitution of the air'), spreading diseases will extend *per se* through it as a medium. Yellow-fever does this, and travels about forty feet a day, and thus, by the concurrent existence of a line of personal communication, spreads over a city and its suburbs, and even beyond. Small-pox has been known to spread

from certain centres (hospitals, for example) for over one thousand yards. It is shown lately (see *The American Journal of the Medical Sciences* for July, 1887, p. 300) by the 'Report of the Local Government Board of England' that from small-pox hospitals the *materies morbi* of *variola* will travel *per se* through the air to the extent of at least half a mile.

I would refer you to the *Medical News*, Nov. 26, 1887, p. 627, for a short but excellent paper on the contagion of scarlet-fever. From its tenor the contagious nature of scarlet-fever is admitted, but it is assumed that it is caused by a germ or living organism which has not been shown to exist. I am one of a small minority among the medical profession who cannot accept the germ theory of disease. Germs of microscopical organisms may, nay, no doubt do, carry contagion, but have no etiological relations as to the primary cause, or *causa causans*, of contagious diseases; and I say this in spite of my familiarity with the literature of the subject, — the latest experiments of Pasteur, Koch-Klein, Edington, Jamieson, Dale, Unger, and others are right now in arm's reach of me, and have been carefully read by me, several for the third time, — I cannot yet accept the germ theory of disease.

[G. C. ASHMAN, M.D., Cleveland, O., health-officer.]

1. I do not. But I think it possible, and even probable, that some of the lower animals have scarlatina, or a disease of which scarlatina in man is a modification.

2. None whatever.

3. (a) An instance where clothing worn by children having the disease in January was brought into another family the following April, cases occurring within ten days thereafter; (b) an instance of school-books used in a family suffering from scarlatina in June, upon being opened and used by other children the following September, appeared to cause the disease; (c) a large number of instances where physicians and others coming in contact with cases have appeared to carry a germ of disease to their own or other families.

4. None.

5. I do not know. My observation leads me to the conclusion that the disease is communicable at any stage after fever begins, and at least until desquamation is completed; and this last stage is often very prolonged.

6. As in No. 3.

7. Three to four months.

8. Most certainly. By the physician or other person making diagnosis. For the prevention of extension, thereby saving life and health. As it is a disease of childhood chiefly, prompt notification of school authorities enables them to exclude infected or infectious children from schools. A placard upon the infected house notifies all who are about to enter of the nature of danger to which they are exposed. It educates the people, and favors isolation.

9. To notify the public of danger, and to render the infected such assistance as may be necessary; to have, if possible, a hospital to which all cases not otherwise isolated should be removed promptly; to give information as to the nature and prevention of the disease.

10. Yes, prompt and complete isolation of every case.

11. An instance of a child in a family of four, none of whom had had the disease; the child affected at once isolated in the same house; the skin in every part washed twice a day with a solution of mercuric chloride (1 to 1,000), and all secretions and excretions treated by the same solution; the isolation maintained for nine weeks; no communication in any way.

12. Yes, in a measure. I believe there is in every individual a natural resistance to diseases, varying in individuals and the same individual at different times, and in respect to certain diseases. This natural resistance is at its best when all bodily functions are best performed.

13. Text-books can do it better.

[To be continued.]

BOOK - REVIEWS.

Higher Grounds. Hints toward settling the Labor Troubles. By AUGUSTUS JACOBSON. Chicago, McClurg & Co. 16°. \$1.

THIS is a small book, as books go nowadays, for it may easily be read through at a sitting. But it demands comment out of

all proportion to its size, for it is both original and powerful. The author's style is clear, crisp, and concise, and, as we shall show by some quotations, is very striking and attractive. Take this as a specimen: "Everybody wants to settle the labor question, but nobody is willing to sacrifice any thing to settle it; nobody appears to be willing to pay out any money to settle it. The labor question will not be settled without sacrifice; it will not be settled without a large expenditure of money. To settle the labor question without sacrifice, would be to get something for nothing. The settlement of the labor question will in some way have to be paid for." The one hundred and sixty pages of the book are full of just such epigrammatic passages as that.

Mr. Jacobson's line of argument is this. The so-called labor problem is the great problem, not only of our time, but of all times. It cannot be settled without expense, and large expense at that. Although any thing but settled, it has already cost this country hundreds of millions of dollars in the way of disturbance of business. The demand of the man who is at the bottom for better things in life is in the nature of things. It is a demand which sooner or later must be met, and it is to the interest of every one that it should be met. The man who works with his hands sells by the quantity, and at the lowest possible prices, all he produces. Whatever he has to buy he buys at the highest retail price. In the game of life the cards are stacked against the man who labors with his hands. Nearly all the wealth of modern times is earned by steam, which does for man his work. The wealth which steam earns should belong to all mankind; but, instead of going to the many, it goes to the few, and the many continue to hopelessly drudge and slave on. But the labor question is not merely a question of fewer hours and more pay: it goes deeper than that. It can be settled, but it can be settled by nothing short of revolution. This revolution, however, will be peaceful: there will be no lawlessness, no destruction of property, nobody would be maimed, nobody would be killed. The revolution is to be effected through the manual-training school.

In June, 1886, the Chicago Manual-Training School sent an entirely new product into the world. It graduated a class of boys about eighteen years of age, who three years before had never touched tools with a view to becoming skilled in their use. Yet, without neglecting their books, they had in those three years drawn the plans for several steam-engines, had made the patterns in wood, had done the chipping, filing, and lathe-work (the casting was done elsewhere, because the school lacked facilities for doing it), had put together the engines, and had run them. This manual education was not special, but general. It mattered not what profession the boys chose: it was useful to them. The product of the manual-training school does not compete with the wage-earning masses, for his skill and intelligence raise him above them. The manual-training school is popular, and destined to become still more so; for at the present time the school facilities that exist for the children of the laboring people, after they are ten or twelve years old, are only a hollow mockery. The manual-training school should become part of the American school system; and, to enable all children to get the benefit of the school, parents or guardians should be paid for keeping the children at school. The compensation should begin at the child's twelfth year, and be fifty dollars per year: it should continue till his twentieth year, when it should be three hundred dollars a year. The proposition includes both boys and girls. The expense thus incurred would be enormous, and could not be met by any taxation now in vogue. It could be met, however, by a graduated tax upon estates. In war times we had a succession tax, and it never failed to be collected, simply because the probate judge could declare no estate settled until the tax had been paid. The law imposing this tax was passed in 1861, and amended in 1862. Both acts were signed by Abraham Lincoln. In war times we also had a graduated income tax; so that a graduated tax is not new to the American people. But the income tax was odious, because only the scrupulous paid, and the unscrupulous escaped by swearing falsely. A succession tax is a fair tax, because nobody can escape paying it. The State of New York has a graduated succession tax, passed by the Legislature of 1885. It is now proposed to enact a graduated tax for all estates. It should be $\frac{1}{4}$ per cent on estates less than \$25,000, $\frac{1}{4}$ per cent on all above

\$25,000 and less than \$50,000, $\frac{1}{2}$ per cent on all above \$50,000 and less than \$100,000, then increasing gradually until it becomes 10 per cent on estates above a million, and 50 per cent on estates above five millions. The tax would not fall heavily upon anybody. It would only be \$2.50 on an estate of \$1,000, and only \$1,000 on an estate of \$100,000. It would yield from twenty-five to fifty millions annually in New York City, and from three to six millions annually in Chicago. Its proceeds would be ample for the proposed expenditure. This measure is expedient, and as just as any tax measure can be. It would increase individual power and individual intelligence, and would produce no unhappiness or suffering. The very rich man might say that he should be permitted to do what he likes with his own; but this he cannot do now, and in the nature of things will never be allowed to do. The law interferes at every step, and tells him what he may do with his own, and what he must not do. Many rich men give as much now for benevolent objects as the succession tax would take from their estates; but they rarely give it so as to accomplish the most good. Untold benefits would arise from this law. The child-labor problem would be settled, because it would now pay to keep the children at school. Wages would rise, because the competition of thousands under twenty years of age, who are now laborers, would be withdrawn. Intelligence shall make the people strong, the people shall be the government, and the strength of the people shall be the strength of the government. This is, in briefest outline, Mr. Jacobson's argument. Its novelty, its brilliancy, and its apparent cogency, nobody will deny; but will it stand the test of practicability? We are compelled to ask this question seriously, because Mr. Jacobson's proposition is made thoughtfully and in good faith: it is not the vagary of a fanatic, or the raving of a lunatic. It is a business proposition, and as such it demands fair treatment.

The second half of Mr. Jacobson's book is devoted to an explanation and eulogy of the manual-training school. We will grant for the sake of argument — though we agree with Mr. Jacobson in the matter, and therefore really grant nothing — that the manual-training school will accomplish all that is claimed for it by its thoughtful and influential advocates. It is then necessary to consider obvious objections to Mr. Jacobson's plan. So far as the graduated tax on estates is concerned, it is an excellent, safe, and easily collected tax. And while it may at first sight seem absurd to talk about subsidizing parents to keep children at school, yet we must take other considerations into account. It is just as absurd, and no more so, to levy a school-tax upon a man who has no children to be educated. Furthermore, it is an expense incurred in order to solve the labor question, which Mr. Jacobson holds, and rightly, cannot be solved without sacrifice and expense. All this being acceded to, it remains to ask whether the proposed tax would pay the bill. The author claims that it would, but offers no statistics in support of his assertion. An examination of the census of 1880 may enlighten us. Mr. Jacobson proposes to pay certain fixed sums per annum for each child over twelve and under twenty. In 1880 there were 8,347,731 such children in the country. To be reasonably successful, Mr. Jacobson's plan should reach at least three-fourths of them. If it should reach so many, and payments were made at the rates laid down by the author, the disbursements under this head in 1880 would have amounted to the enormous sum of \$919,502,737.50, or nine times more than our present total outlay for educational purposes. In the census year there died 756,893 persons. Of these, 202,806 were children under five years of age, whose opportunities of accumulating fortunes were restricted. But let us suppose that one-half of all those who died, or 378,447 persons, left estates valued at \$1,000 and over (we are construing these figures in as liberal a spirit as possible toward Mr. Jacobson, and if we err it is not against his theory). Suppose now, and it is almost unreasonable to suppose so, that 370,000 of these left fortunes averaging \$5,000, that 8,000 left fortunes averaging \$25,000, that 400 left fortunes averaging \$100,000, and that 47 left fortunes averaging \$1,000,000. If Mr. Jacobson's graduated succession tax were levied on these estates, it would net \$10,830,585, making no deduction for cost of collection. And this \$10,830,585 seems paltry when brought face to face with \$919,502,737.50. Mr. Jacobson claims, however (p. 44), as was quoted above, that the tax would at present yield from three to six millions annually in Chicago, and from twenty-five to fifty

millions annually in New York City. We cannot understand on what he bases that assertion. Take the two cities separately. The Illinois counties of Cook and Lake, that in which Chicago is situated and its neighbor on the north, lost 11,433 inhabitants by death in 1880. Of these, 6,230 were infants under five. If, of the remaining 5,203, 5,000 had died with average estates of \$5,000, 175 with \$100,000, and 28 with \$1,000,000,—a most preposterous assumption,—then in those two counties Mr. Jacobson's tax would have netted \$3,075,000. But the conditions are impossible. It is the same with New York. In 1880, New York and six adjoining counties had 25,239 deaths of persons over five years of age. Making an assumption regarding their estates as preposterous as that made in the case of Chicago, the return from Mr. Jacobson's tax would have been less than \$7,000,000.

Two things are very evident,—first, that Mr. Jacobson made no estimate of what his plan would cost; second, that he very much overestimates the number of fortunes of \$20,000,000 and over, in this country. His tax is 50 per cent on fortunes of \$5,000,000 and over, to be sure; and, if a few persons possessing that sum were to die at once, the return would be far greater than we have estimated. But such persons do not all die at once, and moreover, in the long-run, our overestimate of the number of millionnaires would suffice to make up the sum their deaths would contribute. It might even happen that Mr. Jacobson's estimate of the number of immense fortunes is approximately true: the amount raised by the tax would still be far short of the necessary expenditure. The plan is a brilliant one. It has many excellent points. We admire its author's enthusiasm for the manual-training school. His suggestion as to a graduated tax on estates commends itself to our judgment. But as a plan to solve the labor problem, it will not work. This is partly because the income under the plan would not pay the expenditure, and partly because the labor problem is, in many respects, the problem of human nature. In Mr. Jacobson's sense of the word "solution," it cannot be solved.

NOTES AND NEWS.

THE annual meeting of the Association of the Colleges of Ohio will be held at Athens, Dec. 26, 27, and 28, 1887. The following is a list of the papers expected: Monday, Dec. 26, opening address, by Pres. Eli T. Tappan, commissioner of common schools. Tuesday, Dec. 27, 'The Aim of the College,' by Prof. C. L. Ehrenfeld, Wittenberg College; 'Rhetorical Studies and Literary Work in College,' by Prof. W. B. Chamberlain, Oberlin College; 'The Claims of Classical Archæology on Classical Teachers,' by Prof. B. Perrin, Adelbert College; 'Geology and Mineralogy in our Colleges,' by Prof. J. F. James, Miami University; Symposium, 'The Elective System with Us, What we Do and What we Think,' by the presidents or other representatives of all the institutions in the association. Wednesday, Dec. 28, 'Preparation for College in Ohio,' by Prof. Charles Chandler, Denison University. Meetings of the association will be held in the chapel of the Ohio University; entertainment at the Central Hotel, at \$1.50 per day, and at the Warren House at \$2 or less, according to the number stopping there. Trains leave Columbus for Athens at 7.45 A.M., 3.10 P.M., and 6.10 P.M., standard time.

—A literary and musical entertainment was given at the residence of Mr. and Mrs. H. Herrman in New York on Wednesday evening, Dec. 7, in aid of the Erminnie A. Smith memorial prize fund at Vassar College. The evening was a very enjoyable one, there being two hundred and fifty persons present, and a fine collation being furnished by Mrs. Herrman.

—The five lessons on problems in physical geography delivered by Prof. W. M. Davis, under the auspices of the Teachers' School of Science of the Boston Society of Natural History, during the winter of 1886-87, were so novel and useful to teachers, that he has been invited to give a course during the coming winter upon the physical geography of the United States. This course will be in part a continuation of last year's lessons; but the addition of new matter, new models, more extended illustrations, and the special attention given to our own country, will make the lectures practically distinct from those given last winter.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith. Twenty copies of the number containing his communication will be furnished free to any correspondent on request. The editor will be glad to publish any queries consonant with the character of the journal.*

Conspiracy of Silence.

THE discussion published in a recent number of your journal (*Science*, x. No. 252), relative to the faith of scientists, is the revival of a topic which seems to have been long since definitely settled. If history can be credited, scientific men in every age have fought vigorously against progress. An interesting example is furnished by a brilliant French novelist, Mr. Paul Féval, and probably few will fail to recognize the truth of the following quotation:—

"Il fallut cependant des années encore pour que ce savant et illustre corps, le marine de l'Etat, voulût bien prendre en considération cette force qui fait reculer le vent et se rit de la violence même des courants. Il est vrai que l'Académie professait, vers le même temps, cette opinion: qu'une vitesse de dix lieues à l'heure, sur un chemin de fer, supprimerait la respiration chez l'homme et tuerait tous les malheureux assez fous pour se livrer à ces folles expériences. Il serait puéril d'accuser notre marine ou nos académies. Le monde est ainsi fait. Tout progrès gêne quelque intérêt ou froisse quelque orgueil.

"Dans le doute, abstiens-toi, disait la sagesse antique; la sagesse moderne répond: *Si tu ne sais pas, empêche!* Fera-t-on jamais le compte des hommes et des idées mis à mort au nom de ce fantôme idiot que les sages nomment *l'invraisemblance?*"

The naïve confession of Mr. Bonney practically concedes the whole case. Here are two theories of the formation of coral reefs, each dependent upon a certain set of facts, accessible to all investigators. Mr. Bonney says that the scientific method is to wait, and not to investigate. He is not able, he says, to make up his mind which theory is correct. Is this really a scientific method?

The ideal scientist, it will readily be admitted, is a person whose sole aim is to discover the truth of any matter under investigation, regardless of all personal or partisan feelings. The actual man of science, for the reason that he is a man, is influenced, unconsciously it may be, by his human characteristics, and frequently allows prejudice to overcome reason. In the particular case already considered in your columns, it appears that Mr. Murray discovered some facts which were unknown to Darwin, and that, these facts admitted, Darwin's theory must necessarily be modified. This is the precise point which Mr. Bonney adroitly evades: does he believe the facts stated by Mr. Murray; and, if so, can he reasonably continue to accept Darwin's theory? What excuse is there for waiting, unless, indeed, Darwin is an idol whose sayings, because they were made by him, must be received with reverence by all his followers?

This theory of Darwin's is only one of a number of beliefs which scientists uphold with obstinacy, in the face of contrary evidence; but, as is well said by the writer already quoted,—

"Mais, en tout siècle, les sages eurent beau se coucher au travers de la grande route où marche l'humanité, l'humanité passa. L'invraisemblance, grotesque épouvantail, recule ses brouillards devant la lumière. Des miracles, déclarés impossibles, se promènent paisiblement dans nos rues. Et tout va vite; voyez! il y a de cela quarante ans à peine; en cherchant bien, vous trouverez certes encore, vivant et grignotant sa bribe du budget, quelqu'un de ces Spartiates dont la main tremblotante essaya d'arrêter la vapeur!"

It may be of interest to glance briefly at another celebrated theory, which has been treated by scientists in a manner very similar to that pursued in the case of coral formations. About the year 1844, Messrs. Favre and Silberman experimented on the heat evolved by the combustion of certain elementary and a few compound combustibles. Their experiments, far surpassing in accuracy all those hitherto made, were accepted by scientific men generally, and their results are given in most text-books and treatises on heat. These distinguished experimenters did not think it necessary to test the heating-power of the familiar compound, coal, but considered that it could be calculated with sufficient accuracy by analyzing the coal, and assuming that the heating-power was the same as the sum of the heating-powers determined for the various combustible elements, less the unavailable heat of so much of the

hydrogen as would unite with oxygen in the formation of water; and this is the theory contained in most modern text-books on combustion prepared for the use of English-speaking students, and generally employed in calculations by the scientific men of England and the United States.

About the year 1860, Messrs. Scheurer-Kestner and Meunier-Dollfus made experiments on the heat evolved by the combustion of various coals; using the same method as that employed by Messrs. Favre and Silbermann, and checking the latter's experiments on wood charcoal and hydrogen gas, before testing the coals. The experiments on coals showed that it was incorrect to calculate the heat of combustion of coal from the heat of its combustible constituents as determined by Favre and Silbermann; or, in other words, that it was not correct to assume that the carbon in coal was of the same density as wood charcoal, and that the hydrogen of the coal was in a gaseous state: these being the necessary assumptions, when Messrs. Favre and Silbermann's constants are used in the formula to which reference has been made above. The report of Messrs. Scheurer-Kestner and Meunier-Dollfus has been well named 'classical,'—all operations and calculations being fully detailed,—so that, speaking rationally or scientifically, the conclusion seems inevitable that a scientific investigator must either find some error or accept the results. Well, how has the scientific world, that is to say, the English-speaking scientific world, received these results? Generally by ignoring them, and going on in the good old way, according to the creed formulated by Messrs. Favre and Silbermann. Here is a scientific (?) statement made by one investigator who has carefully studied the report (Mr. B. F. Isherwood, in *Journal of the Franklin Institute*, July, 1884):—

"The results of the calorimetric experiments made by Scheurer-Kestner and Meunier-Dollfus on the heat of combustion of the Alsatian coals, were never accepted by the British scientists, notwithstanding that no error was ever pointed out in either the apparatus or the method employed. Nor could the writer ever accept them, although he bestowed the closest scrutiny and study upon them."

This is science, with a vengeance! "I can't find any mistakes in the methods or calculations," says the scientist, "but the results are opposed to my present belief, and I can't accept them. I have published numerous treatises containing calculations founded on the methods and data of Favre and Silbermann, and these new results, which would condemn my work, must be ignored or denied."¹

The statement contained in the above quotation, that the results of the experiments made by Messrs. Scheurer-Kestner and Meunier-Dollfus were never accepted by the British scientists, is not absolutely correct. Mr. John Percy, in the last edition of his treatise on fuel, gives the results, and calls attention to the inaccuracy of the ordinary method of calculation. Similar corrections are made in the last supplement of Watt's 'Dictionary of Chemistry.'

Quite recently, Messrs. Scheurer-Kestner and Meunier-Dollfus have repeated their former experiments, obtaining substantially the same results as before; and it seems probable that right methods of calculating the heat of combustion of coal will be generally adopted before long. If the results are true, they will certainly be accepted, some day; but the length of time during which they have patiently awaited admission to the temple inhabited by English-speaking scientists is a sufficient answer to the question, 'If a new fact, overturning some established theory, is presented, do the scientists examine it critically, and either disprove or accept it, or do they ignore it as long as they can, and only take it into their hearts when worn out by its persistent demands?' If a *truth* is announced, there need be no fear that it will not prevail in the end; but numerous facts, similar to that just cited, sufficiently disprove

¹ Scientific men seem to hunt in couples, so to speak; and Mr. Bonney, in answering the Duke of Argyll (*Nature*, Nov. 24), argues in the same manner as his American brother in the above quotation. He does not accept the new theory, and thinks that no reasons are required for his disbelief. Here are his words:—

"To conclude, the Duke still—and this is our special complaint—treats the matter rather according to ecclesiastical than to scientific method. He is fully persuaded of the excellence of Mr. Murray's hypothesis, and considers it to be 'one of those discoveries in science which are self-luminous,' and 'must carry conviction to all.' Very well, but there are some people, not very few in number, who do not share this opinion."

Hail to the new science, announced by Mr. Bonney! The voice of many people is the voice of God.

the theory fondly entertained by many scientists, that they have reached the ideal state where they desire only to know the truth, regardless of consequences.

Another brilliant French writer, Mr. Alexander Dumas, well sums up the matter, as follows:—

"Il est vrai que peut-être les contemporains ne me croiront pas. . . . Qu'importe! je l'aurais dit; d'autres me croiront: la vérité est une de ces, étoiles qui peuvent rester des mois, des années, des siècles, dans les profondeurs du ciel, mais qui finissent toujours par être découvertes un jour ou l'autre. J'aime mieux être le fou qui se voue à la recherche de ces étoiles-là, que le sage qui salue et qui adore, les uns après les autres, tous ces soleils que nous avons vus se lever, que l'on nous a donnés pour des astres immutables, et qui, à tout prendre, n'ont jamais été que des météores plus ou moins durables, plus ou moins brillants, plus ou moins trompeurs, toujours fatals!"

RICHARD H. BUEL.

New York, Dec. 7.

The 'Act of God' and 'Fuerza Mayor.'

MR. APPLETON MORGAN'S 'Act of God' and Mr. Nevin's 'fuerza mayor' appear to me to be pretty much alike, and to threaten a new peril to railway travel,—a peril, according to Mr. Nevin, which in Mexico is already to be encountered. I tremble to think what might happen, for example, if the engineer of the locomotive should happen to sneeze just as he passed a signal that a bridge had been carried away somewhere on the Mexican Central Railroad by "the flooding of a river." Here would be a double 'fuerza mayor'; for an inclination to sneeze is certainly irresistible, and, besides, "the flooding of a river" certainly relieved from the responsibility for the irresistible inclination, even if, according to Mr. Nevin, it did not relieve the watchman from the duty of putting up the danger-signal. But, although we may have to take our lives in our hands when we travel by rail in Mexico (according to Mr. Nevin), I hope that time has not yet come in the United States.

In short, this is the actual practical answer to Mr. Morgan's cleverly reasoned and delightfully *insouciant* paper. It may not be the answer a railway lawyer would write, or would recognize as sufficient, but, from the travelling public's standpoint, it is all there is to be said. It is all very well for the sleek attorneys of great railroad corporations to say that so long as the company provides, as Mr. Morgan says, "the last improvement in safety-insuring devices," its responsibility for the safety of those it transports ceases. "Let us bow to the Divine Will, gentlemen of the jury," says Mr. Morgan. "An overruling Providence has decreed that my client should 'roast thirty-two human beings in slow agony on a floor of ice at White River. But our track was in perfect order, our engine was all right, we were running on time. We are not legally to blame.'" Would Mr. Appleton Morgan have bowed to the Divine Will if he had happened to have been rescued in a half-roasted condition at White River, less an arm, or an eye, or a leg? I venture to say he would have done nothing of the sort. I venture to say he would have commenced proceedings against the company for twenty-five thousand or fifty thousand dollars as soon as he could swear to a complaint. And yet Mr. Morgan will concede that the accident at White River could not have happened in spite of the Divine Will.

The people of this nation do not exist at the will and pleasure of the railway-companies; nor is this nation governed by Mexican laws. Mr. Morgan's familiarity with his subject enables him to write very plausibly concerning the rights and duties of railway-companies; but he cannot convince me, for one, that they are not more sinning than sinned against. If the principle of the 'Act of God' is to be resurrected in the United States, as in Mexico, where is the line to be drawn, and who is to draw it,—the railway-companies, or their ingenious lawyers?

GEORGE BRADWIN.

Jersey City, Dec. 6.

The Flight of Birds.

My friend, Prof. Frank H. Storer, has called my attention to an important note on the wings of birds, by that accurate and indefatigable investigator, Prof. Jeffries Wyman. It is to be found on p. 169, vol. v., *Proceedings of the Boston Natural History Society*. This note is all too short, but forms an interesting adjunct to the

paper by Professor Trowbridge, read before the National Academy, and noticed in *Science* of Nov. 18, 1887. Those who heard or have read Professor Trowbridge's paper will remember that it reported the discovery by his son of a peculiar structure in the primary wing-feathers of soaring birds, by which they are locked when expanded, and are thus maintained in position without muscular effort. This structure is shown only in the primary feathers, and is therefore a character belonging to the last division of the arm.

Professor Wyman, in the note referred to, describes "a peculiar arrangement of the bones and ligaments in the wing of the pin-tailed duck, by which, while the wing is fully extended, all the segments of this extremity are fixed and retained in position independently of muscular action." His account of the mechanism of the wing is as follows:—

"The structure of the articulations of the elbow and wrist is such, that during flexion and extension the radius advances and recedes upon the ulna, carrying with it the upper carpal bone, and this last the hand: in this way flexion and extension of the bones are effected. The lower carpal bone is attached to the upper by strong ligaments: consequently, when the upper carpal bone is drawn over the extremity of the ulna as the radius recedes, the lower one is drawn up between the hand and the extremity of the ulna, and, acting as a wedge, maintains the hand extended, until it is displaced by the reversed action of the radius." This structure, according to Professor Wyman, shows how the extension of the bony framework of the wing may be maintained indefinitely without fatigue. The structure of the primary wing-feathers described by Professor Trowbridge indicates that they too may be locked in position, and thus the rigidity of the wing may be maintained automatically to its extremity.

None of the members of the National Academy who took part in the discussion which followed the reading of Professor Trowbridge's paper seemed to have any knowledge of this discovery of Professor Wyman; and it was remarked, that, while the facts cited by Professor Trowbridge seemed to explain the automatic extension of the primaries which are appendages of the *manus*, the rigidity of the arm itself, apparently manifested in the flight of soaring birds, was yet unaccounted for. That missing link was supplied by Professor Wyman, but, from his characteristic modesty, so quietly announced that it has been known to few.

The case cited by me at the meeting of the National Academy was a turkey-buzzard, shot when soaring over the prairies in the Sacramento valley. Its wings remained rigidly extended, and it descended slowly like a parachute and settled in the grass very near me, quite dead; even then the wings remaining expanded.

Professor Storer gives other interesting examples. He says, "Upon the New England seaboard nothing is more familiar to old gunners than the phenomenon that a bird shot in mid-air will often 'set his wings' and scale down toward the horizon, to reach the water dead, often at a great distance from the boat whence the shot was fired. Even in childhood I remembered to have wondered, when 'assisting' at the shooting of duck and coot, as to the meaning of the not infrequent exclamation, 'That fellow has set his wings; watch him!'"

Dr. Storer writes that he was present at the meeting of the Boston Natural History Society, Sept. 1, 1855, when Professor Wyman exhibited his preparation of the duck's wing, and gave an explanation of its structure which seemed a demonstration.

Now, if some good anatomist would review the subject again, combine the results reached by Professor Wyman and Professor Trowbridge, and illustrate his memoir with good figures, he would make an important contribution to biological science.

J. S. NEWBERRY.

New York, Dec. 10.

The Origin of the Tritubercular Type of Mammalian Dentition.

PROFESSOR COPE has fully demonstrated that the molar teeth of many divisions of the higher mammalia are derived from the tritubercular type of molar which is so abundant in the mammals of the Puerco, or lowest eocene period. He has further ('Origin of the Fittest,' p. 347) shown that the tritubercular type may be traced back to the single cone of the reptilian crown by the follow-

ing succession: 'first, a simple cone or reptilian crown alternating with that of the other jaw; second, a cone with lateral denticles; third, the denticles to the inner side of the crown forming a three-sided prism, with tritubercular apex, which alternates with that of the opposite jaw,' etc. In the last meeting of the American Association for the Advancement of Science, Professor Cope applied this succession to the origin of what he has called the 'tubercular-sectorial' molar, citing the molars of Owen's genus *Spalacotherium* as an instance of the transformation into the tritubercular crown *in process*. I had independently arrived at the same conclusion, and, moreover, found that the origin of the tritubercular crown in all its various stages could be traced in the mesozoic mammalia. This is traced in a memoir now publishing by the Philadelphia Academy. I am glad to be able to confirm Professor Cope's views in every particular, for his numerous and suggestive papers upon the mechanical genesis of tooth forms have placed comparative anatomists generally in his debt. Among the mesozoic mammalia the simple large cone with small lateral denticles is seen in the American triassic genus *Deomotherium*. From the same beds *Microconodon* furnishes a more advanced stage in the growth of the lateral denticles into cusps. The mandibles of Jurassic genera *Phascolotherium*, *Menacodon*, *Spalacotherium*, furnish three stages of the rotation inwards of the lateral cusps, accompanied probably by the rotation outwards of the lateral cusps in the upper jaw. In *Stylodon* this process is complete, the teeth being distinctly tritubercular, with the addition of a posterior heel, the upper molars reversing the pattern of the lower. In another line of genera the lateral cusps show no tendency to rotate inwards, but continually augment in size, such as *Triconodon* and its successors, leading to the modern *Thylacinus* type of molar. In *Amphitherium* and many other genera it appears as if the posterior lateral cusp had never been acquired, and the crown is re-enforced by the inward extension of the cingulum, as seen in an early stage in *Diplocynodon*. In *Kurtodon*, by the union of the external tubercles in the upper jaw, we observe a columnar molar of the rodent type. It now appears as if we should soon be in possession of sufficient data to trace the entire history of the multi-cuspid and multi-fanged mammalian molar from the single reptilian cone and fang.

HENRY F. OSBORN.

Princeton, N. J., Dec. 12.

Iroquois and Eskimos.

IN connection with the discussion which has recently appeared in *Science* on the ancient relations of the Iroquois and Eskimos, a passage which I recently came across in the manuscripts of the Moravian missionary Christopher Pyrlæus is worthy of note.

The active work of Pyrlæus was between 1740 and 1750, and he became an accomplished scholar in one or more of the Iroquois dialects. In July, 1749, the Iroquois sent a deputation to a council at Philadelphia, when Pyrlæus acted as interpreter. In his notes of his conversations with the deputies he has the following:—

"*Tschiechrohne* heissen die Grönländer; . . . *Tschie*, ein See-hund. Die drei obgenannte Seneker wussten nicht nur von den Grönländern, sondern auch ihrer Contry (*sic*), Landsart, Kleidung, Nahrung," etc.

Of course, Pyrlæus used the term 'Greenlanders' as generic for 'Eskimos.' Evidently the Iroquois, who pushed their war parties to the south as far as the present State of Louisiana, carried their excursions also as far north as the shores of the Frozen Ocean.

D. G. BRINTON.

Media, Penn., Dec. 7.

The Sioux.

IN your issue of Nov. 25 (p. 264) your correspondent from Lexington, Mo., says, "The Sioux call themselves *Lah-ko-ta*." In this he is correct; but when he adds, "not Dakota," he is in error. The sounds of *l* and *d* are interchanged among certain Dakota dialects. The Sioux who dwell east of the Missouri say *Dakota*, while most of those on the west side (Tetons) say *Lakota* (*vide* Riggs's *Grammar and Dictionary of the Dakota Language*, p. 133).

In giving the meaning of the name as 'cut-throats,' he is at variance with the best authorities on the Dakota language.

W. MATTHEWS.

Washington, D. C., Nov. 26.